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A REPLY TO DR. F. N. OTIS.

BY

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fessor of the Practice of Surgery in the College of Physicians and Surgeons, New York; Attending Surgeon to the New York and Roosevelt Hospitals.

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n article appeared in THE HOSPITAL GAZETTE of April 19th, in
ch Dr. F. N. Otis criticises a lecture on Spasmodic and on Inflam-
ory Stricture, published by me in the same journal of February
. The article contains so many misstatements, that I feel desirous
orrect them, and to offer a few words in defence of the ground I
e taken in the controversy between us. I will endeavor to notice,
atim, the points in Dr. Otis's paper which I wish to make the sub-
of either comment or explanation.

Dr. Otis expresses much surprise that his peculiar views concern-
spasmodic urethral stricture, which he had so often and so stren-
sly advocated during the past six years, had excited "no public
ce;" and says he had begun to hope that his "formidable array"
acts had been "quietly accepted" by "all fair-minded and intelli-
t surgeons." His surprise is more reasonable than his hope; for,
le it is true that his views are not even mentioned by standard
gical authors, such as Van Buren and Keyes, Ashhurst, Thomp-
, Holmes, Erichsen and Bryant—all of whom have published
tises on surgery since these views were set forth—this very fact
ht perhaps indicate that they had been quietly ignored, rather than
etly accepted, by the more sober-minded representatives of our
fession. The lecture which I published, was intended especially
guard the younger medical men against the effects of what I hon-
y believed to be unsound teaching, the influence of which I
ught might prove highly injurious, if allowed to go without a
test.

2. I am charged with having erroneously ascribed to Verneuil, the invention of a theory which originated with Civiale. But, what theory? Dr. Otis says, "the theory of reflex action, applied to urethral difficulties"—a somewhat vague, though comprehensive phrase. The credit of priority in this matter seems to have been to Dr. Otis a kind of stumbling block. At first he thought himself entitled to it; long afterward, through the "careful search" of an "accomplished friend," he finds that he was anticipated nearly thirty years ago by the celebrated Civiale, with whose classic writings he tacitly admits that he first became acquainted in May, 1878. An awkward confession, truly, from a Professor of Genito-Urinary Diseases! He then offers an humble apology to Civiale, as the one who first advanced the "theory of reflex action" as "applied to urethral difficulties," although a little careful study would have taught him that Civiale neither deserved nor claimed any credit for originality in this respect. Indeed, Civiale does not employ the phrase "reflex action," but uses the word "sympathy," as it was used by the older writers, to denote the causal relation between morbid affections having their seat in parts of the body more or less remote from one another. These reflex, or sympathetic, disorders connected with the genito-urinary apparatus did not escape the notice of Van Swietan, who, more than a century ago, observed that calculi, when arrested in the ureters, might cause such irritation of distant parts as to mask the primary disease. He reports the case of a man in whom the descent of several small calculi caused, as the earliest symptoms, pain in the scrotum and testicle, and afterward in the back part of the ilium. Chopart, [1] when describing the effects produced by the presence of a calculus in the ureter, says, "The irritation is not confined to the ureter, but reaches to the kidneys, the bladder, and the urethra; it extends often, indeed, to the spermatic vessels, the testicles, and along the thigh." John Hunter, writing in 1786, concerning irritation of the bladder as a sequel to gonorrhœa, remarks: "The irritation of the bladder sometimes continues after every other symptom has ceased. * * * It may arise

[1] Maladies des Voies Urinaires, Paris, 1830, vol. I., p. 309.

rom its connection with other parts, such as the urethra or prostate land, for a stricture in the urethra coming on will prove the cause f its continuance," etc[1]. Again, "I have seen a chancre of the repuce produce a pain in the urethra in making water, which most robably depended upon a sympathy similar to that by which the pplication of venereal matter to the glans produces a discharge from he urethra, as was observed above." [2] Also, "I have known the rethra sympathize with the cutting of a tooth, producing all the ymptoms of a gonorrhœa." [3]

Edward Home, in 1803, describes "Sciatica in consequence of tricture," and relates at length two cases, in both of which the ervous affection disappeared upon the cure of the primary disease.[4] He also devotes a section to "Strictures producing a diseased state f the surface of the tongue, which disappeared upon the removal of he stricture."[5] Marshall Hall writes in 1847, [6] "A minute calculus ituated high up in the urethra has induced such contraction of the phincter ani, as almost to close the canal. A ligature upon a emorrhoidal tumor has induced retention of urine. In a little boy, he nephew of Dr. Heming, strangury was induced, in the most nequivocal manner, by dentition. The case was supposed to be alculus. It was relieved at once by effectually cutting the gums." astly, Brodie's well-known case deserves to be cited: "A gentleman onsulted me concerning a pain in one instep. The pain was severe, ausing lameness, so that he walked with difficulty; but there was either swelling, nor, except the pain, any mark of inflammation. I rescribed some remedies, which, however, were of no avail. One norning he called on me, still suffering from the pain in the foot, and o lame that he could not get out of his carriage and walk into the ouse without the assistance of his servant. Now, however, he

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[1] A Treatise on the Venereal Disease, by John Hunter. London, 1788, p. 107.
[2] A Treatise on the Venereal Disease, by John Hunter. London, 1788, p, 60.
[3] A Treatise on the Venereal Disease, by John Hunter. London, 1788, p. 33.
4. Home on Stricture, London, 1803, vol. II., p. 271, etseq.
5. *Ibid.*—Vol. II., p. 306, et seq.
6. Memoirs on the Nervous System, London, 1837, mem 2, p. 99.

complained of another symptom; he had difficulty of making water, and a purulent discharge from the urethra. He had labored under a stricture of the urethra for many years, and had occasionally used bougies. Of late the stricture had caused more inconvenience than usual; but he had abstained from mentioning it, thinking that it would be better that he should (if possible) be relieved of the pain in the foot before any treatment was adopted on account of the stricture. Under the circumstances I introduced a bougie, which penetrated the stricture and entered the bladder. Immediately on the bougie having been used, the pain in the foot abated; and in less than a quarter of an hour he left the house free from pain, and walking without the slightest difficulty. This happened some years ago, but I have seen the patient at intervals ever since; and from a most careful examination of his case, he and I are both satisfied that the pain in the foot is connected with the disease in the urethra, and we have never found anything to relieve it except the introduction of the bougie." [1]

All the authors above quoted wrote before Civiale. It is therefore evident that Dr. Otis is not well informed, when he states that "the theory of reflex action applied to urethral difficulties was first advanced by Civiale."

The theory attributed by me to Verneuil is very distinctly stated in my paper. It is that which denies the comparative frequency of deep-seated organic urethral stricture, asserting that what appears to be such, is commonly a contraction of the canal, due to a spasm of the compressor urethræ muscle, such spasm being the result of a reflected irritation from one or more organic strictures situated in the penile portion of the urethra. This theory, which clearly does not belong to Civiale, was, as I have stated, advanced by Verneuil in 1866, seven years before it was announced by Dr. Otis, without any acknowledgement of his indebtedness to the man whom he is pleased to style "Chief of French Surgeons of to-day." Evidently, Dr. Otis is not so

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1. Brodie, Lectures on Certain Nervous Affections, London, 1837, p. 38.

familiar as we might suppose he would be with the writings of one who thus commands his admiration.

3. I am quoted as having said of Folet, that "he had mistaken the triangular ligament for a muscular spasm." This unmeaning sentence was composed by Dr. Otis, and cannot be found in my paper. A writer may be pardoned if he fails to understand the meaning of his opponent's language; but, in pretending to quote it, he has no right to enclose with quotation marks a sentence of his own invention.

4. Dr. Otis ridicules the idea of testing the theory of spasmodic stricture by an appeal to pathological anatomy; and triumphantly inquires what I would consider to be "the pathological anatomy of a spasm?" This question betrays a misapprehension which might easily have been avoided. I have not asked for the anatomical evidence of a spasm, but have simply demanded such evidence in favor of the theory of spasmodic stricture, this theory being based upon the alleged relative frequency of organic stricture in the penile portion of the urethra. So far as we at present know, deep-seated strictures are common, while anterior strictures are rare. The contrary is asserted to be the case by the advocates of Verneuil's theory; and although it may be perfectly true, it cannot be accepted as a scientific fact, in the absence of evidence derived from morbid anatomy. Verneuil himself understood this point very clearly; and that was the reason why he brought the subject before the notice of the Anatomical Society, where, otherwise, it would have been out of place.

I may remark, in passing, that if those who are cutting and curing organic strictures by the hundred, and who seldom see a meatus urinarius which they consider normal, would pay a little more attention to the study of pathological anatomy, they would add weight to their testimony, and obtain knowledge which might induce them to modify their opinions. The frequency with which urethral stricture is said to be met with nowadays, calls to mind the account of a rectal specialist who practiced in the western part of England in 1844, and who claimed to treat so extraordinary a number of cases of stricture of the rectum, as to cause a layman to send a

communication to the *Provincial Medical Journal*, stating that the disease was endemic in the locality where this practitioner resided, and advising strangers to avoid the place, inasmuch as nearly every person who went there became attacked.

5. Dr. Otis is not aware that any surgeon has assumed the association between penile and spasmodic stricture to be invariable I therefore quote for his information the following: "Dans *tous* les cas de rétrécissement pénien, il existe un deuxième arrêt à 13 centimètres du méat, au commencement de la région musculeuse, à l'entrée du col uréthro-vésical." [1]

6. I am blamed for dogmatising on the subject of spasmodic stricture, and for declining to scrutinise the cases that have been reported. On the contrary, I have sought earnestly, but in vain, for clinical evidence in support of Verneuil's theory; while I have been unable to accept the reported cases as being free from errors of observation. In short, we need satisfactory proof, not so much of the theory of spasmodic stricture, as of the "formidable array" of facts on which that theory rests, and without which it cannot claim recognition. To show that I am not fastidious, and to illustrate my meaning, I call attention to the four cases mentioned by Dr. Otis, as having been treated lately at the New York Hospital, in which cases he alleges spasmodic stricture was "proven to exist," and which, he says, "*would have been operated on by the perineal section, if the spasmodic character of the obstruction had not been determined by a previous dilating urethrotomy.*" The cases are thus given by Dr. Otis. "Within the previous month a patient was admitted to the wards of the New York Hospital, suffering from deep urethral stricture. The stricture was a very close one, and located in the membranous urethra. The operation of perineal section was decided upon. Notices to that effect were issued. The patient, when the proper time arrived, was etherized, brought into the amphitheatre of the New York Hospital, and the perineal section was about to be performed.

[1] Étude sur les Rétrécissements Péniens de l'Uréthre: Folet: *Archives Générales de Médecin*: 1867, vol. 1, p. 424.

The operator, a distinguished surgeon and colleague of Prof. Sands, had become familiar with my procedure in such cases, and he proposed, after ætherization, in order to test the matter of diagnosis more fully, to remove, first, several anterior contractions which were found to be present. This was accordingly done with my dilating urethrotome, clearing the penile urethra from stricture, stopping short of the deep stricture at 5½ inches. A large sound was then entered and *slipped by its own weight into the bladder.* A second case, in the service of the same surgeon, of exactly similar character, and two others of exactly the same kind, occurred in the service of another of Prof. Sands' colleagues in the same hospital, within the following two months."

I beg leave to compare Dr. Otis's account of the first case with the following one, condensed from the Hospital Case Book, (Vol. 11, 1878, p. 165,) which is open to public inspection. Bernard O'C., æt. 35, was admitted July 31, 1878. Patient had gonorrhœa nine years ago, the discharge becoming gleety and lasting for six years. In the fifth year of the disease he had a perineal abscess, which healed after remaining open for ten weeks. Another abscess formed at the same site, about four weeks before admission, leaving a fistula which had not yet closed. When admitted, he passed stream of urine about size of knitting needle; examination of urethra detected obstruction about 5 inches behind meatus, admitting only a filiform bougie. At the same point a steel sound. No. 25 F., entered what appeared to be a false passage. High fever, with thrombosis of the left femoral vein, followed this examination, and no further mechanical treatment was undertaken until Sept. 26th, when the deep stricture was found impassable to filiform bougies. The perineal fistula admitted a probe, which passed about an inch upward and backward toward the bladder. Sept. 28. Operation: Patient etherized; flexible bougie, No. 5 F., entered bladder with difficulty, encountering resistance in the perineum; meatus, which admitted No. 25 F., incised, and with No. 22 F., strictures diagnosticated at 2 ½ and 4½ inches from meatus. These were cut with the dilating urethrotome to No. 37, after which

sound No. 35 F. passed without difficulty into bladder. Subsequently, steel sounds—gradually diminishing in size to N. 31—were passed every few days, until patient left the hospital, Oct. 20th, at which time the perineal fistula still existed.

The discrepancy between Dr. Otis's report and the one I have given must at once strike every reader. According to the official record, there is reason to believe that the patient had organic trouble in the perineal portion of the urethra, as evidenced by the signs of a false passage at the point of obstruction, and by the existence of a fistula, which, although it was not proved to have any communication with the urethra, was quite likely the remnant of a urinary abscess. A false passage, if such existed, would be apt sometimes to catch the point of a sound, which at other times might take the course of the urethra, and enter the bladder. At all events, the complications in this case—no mention of which is made by Dr. Otis—render it utterly worthless as a proof of the existence of spasmodic stricture.

The second case is not described, but is stated by Dr. Otis to have been of "exactly similar character" with the first one. Let us examine it in the light of the following facts obtained from the Hospital Book, vol. 11, p. 153.

"James Hughes, admitted Aug. 16, '78, had gonorrhœa four years ago, and for the past two years has had the usual symptoms of organic stricture. Has been treated by dilatation up to No. 9 F. On examination, a close stricture was detected at the bulbo-membranous junction, grasping, but not allowing, the passage of a filiform bougie. Afterward, small instruments entered the bladder, and the stricture gradually yielded, until, at the end of six weeks, it admitted the passage of a steel sound, No. 15 F. At this time, ether was administered, the meatus incised, and three strictures, admitting a bulbous sound, No. 18 F., and situated at 2, 2½ and 3¾ inches respectively behind the meatus, were incised by the dilating urethrotome screwed up to 34. An attempt was then made to pass sound No. 31 into the bladder, but the point of the instrument was arrested by the deep

stricture, through which nothing larger than No. 23 could be inserted. The record then reads, "Determined not to cut the stricture which was far back, but to accomplish the future cure by dilatation." This was done, sounds of gradually increasing dimensions being introduced until Dec. 20th, when the patient left the hospital, the stricture at that time admitting sound No. 30, which caused some pain.

The only exact resemblance which I am able to trace between this case and the preceding one, is in its complete failure to verify Dr. Otis's assertion, that spasmodic stricture "was proven to exist."

I have been unable to find, "within the following two months," the "two others of exactly the same kind," but I presume that I have found, in the records of April and May, 1878,[1] the cases to which allusion is made. These cases are so carelessly written, however, and the facts and figures are so jumbled, that I defy anybody to draw from them any definite conclusion. But, taking the first two cases, I would inquire what reliance can be placed on Dr. Otis's version of them? And if, to this set of cases, can be applied the motto "Ex uno disce omnes," his "formidable array" of facts, will be formidable only to those who may be called upon to endure the heroic operations which they are held to justify.

7. I am charged with being vague in expressing my disapprobation of the operative procedures advocated by Dr. Otis, and am challenged to "attempt the somewhat difficult task of stating the character and amount of damage done." I will therefore endeavor to be more definite. I have frequently seen the operation of slitting the meatus carried to such an extent that the patient afterward was unable to project the stream of urine in the natural manner; and I know of a case in which an eminent surgeon was obliged to perform a plastic operation to restore a meatus which had been destroyed. I have seen in consultation persons who have suffered from troublesome hemorrhage—varying in duration from a few days to a month—in consequence of having been cut with the dilating urethrotome, an excellent instrument of its kind, but the use of which has been carried to a

[1] Hospital Case Book, Vol. 1, pp. 418 and 432.

dangerous excess. Finally, I have heard of a number of cases in which death has resulted from the employment of the dilating urethrotome. It is hard to obtain access to these fatal cases, which are not usually reported, and which are generally considered as a kind of private property. I can state with authority, however, that three fatal cases of operation with the dilating urethrotome have lately happened in our city hospitals, two of which occurred last week in one hospital. In two of the cases mentioned, death took place from pyæmia within a week of the operation. In the third case, death occurred from uræmia on the sixteenth day after the operation, which was performed for the division of an anterior stricture so slight as to be detectable only with a bulbous sound No. 24 F. At the autopsy, three deep incisions were found, involving the anterior three and a half inches of the floor of the urethra, the mucous membrane of which, in this situation, was not thickened, and showed no appearance of disease to the naked eye. A tight organic stricture, undivided, was noticeable at the bulbo-membranous junction. This, during life, had been treated by dilatation.

I think I have said enough to show that the theory of spasmodic stricture, as taught by Dr. Otis, is unsupported by trustworthy evidence, and has led to serious errors in practice. Believing the doctrine to be false, and the practice dangerous, I should feel that I was recreant to my trust as a public teacher, if I failed to oppose the one, or denounce the other.

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